AMERICAN JOURNAL OF PUBLIC HYGIENE
AND
JOURNAL OF THE
MASSACHUSETTS ASSOCAITION OF BOARDS OF HEALTH
ORGANIZED 1890.
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VOL. XVI. (New Series. Vol. II., No. 2) February, 1906. NO. 1

THE MASSACHUSETTS ASSOCIATION OF BOARDS OF HEALTH was organized in Boston, March, 1890, *with the following objects; The advancement of sanitary science; the promotion of better organization and co-operation among local boards of health, and the uniform enforcement of sanitary laws and regulations*

THE JOURNAL OF THIS ASSOCIATION has for fifteen years faithfully reflected the views of the public hygienists of Massachusetts. With the November issue of the year 1904, *the encouragement received from many hygienic quarters induced the Association to undertake the expansion of the Journal to a national standard, with the co-operation of many noted sanitarians as editors. A new title was selected, while the older and well-established title was retained also.*

The dates of publication are FEBRUARY, MAY, AUGUST *and* NOVEMBER *of each year.*

THE JOURNAL will contain the papers read at the meetings of the Association, papers contributed by hygienists, reports of the discussions, editorials, abstracts, reviews and hygienic notes of professional interest. Subscription rates, $1.00 *per year. Reprints furnished at cost price.*

All communications concerning the JOURNAL, copy, proof, subscriptions, advertisements, etc., should be addressed to the Managing Editor, DR. H. W. HILL, 739 *BOYLSTON ST., BOSTON, MASS.*

All communications concerning the ASSOCIATION should be addressed ta the Secretary of the Association, JAMES C. COFFEY CITY HALL, WORCESTER, MASS.

All bills not relating to the Journal, and MEMBERSHIP DUES ($2.00 *per year), should be sent to the Treasurer of the Association DR. JAMES B. FIELD,* 329 *WESTFORD ST., LOWELL, MASS.*

EDITORIALS.

EXECUTIVE SANITARIANS AS SPECIALISTS.

Sanitarians represent an important group of men who are dealing with special problems. As workers, they may be divided into executive, constructive and laboratory sanitarians. Medical men make up a large proportion of the executive sanitarians; engineers a large proportion of the constructive sanitarians. In the laboratories are found men —medical and non-medical—well trained along various scientific lines bearing upon sanitary problems. Some of these continue indefinitely in routine or research laboratory work. Others drift after a time into the executive or the constructive lines of work.

Sanitary problems, as viewed at the present time, are largely of recent origin, belonging to the bacteriological era which is not yet half a century old.

In 1872 the American Public Health Association was organized, and in 1898 it enlarged its field of usefulness by creating a Laboratory Section. In 1885 the Conference of State and Provincial Boards of Health was organized. In 1902 the functions of the United States Marine Hospital Service, which had been enlarged from time to time during the previous century, was still further extended, making of it a National Public Health Service. These organizations all have a national or international standing of a strictly sanitary nature.

In addition to the above, there are various technical organizations which must in the very nature of things deal with sanitary problems. These include the Societies of Engineers, Bacteriologists, Entomologists, Chemists, etc. All of these, so far as sanitary work is concerned, should have some common meeting ground. It is even a question if they should

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not all be parts of one great sanitary organization. At present scientific men are hampered in their sanitary work because of lack of opportunity to compare notes with others working along somewhat similar lines or because they have no opportunity to put to the test, through executive departments, the practical operation of their methods.

If sanitarians are to keep in the front rank they must have able executives as their leaders, in order that the practical scientific work along sanitary lines may be fully developed. What are the actual conditions? There are but few States that are thoroughly organized in sanitary work, and only a few of these give sufficient financial compensation to their executive officers to furnish them a creditable living. Still further, most of the appointments for the executive officers are purely political, and there is no inducement for capable medical men to give up lucrative and pleasant professional work for a temporary political position as a sanitary expert. The same is true of cities. It would seem that in the larger of these, men of ability could and should be secured as permanent officials in such important sanitary work. Yet in the twenty-five largest cities of the United States I venture to say that there are not five sanitary executives who give their entire time to their department, and none who are free from political supervision.

This is a day of specialization. Specialization is necessary if the best results are to be obtained. Encouragement should be given to medical men to specialize in sanitary work, for preventive medicine is of greater importance to the people at large than is curative medicine. No such encouragement exists at the present time. The medical man who chooses to take up a special line of work should feel that he is making an upward step in his professional life. The medical man who is bold enough to step out of general professional work into the position of a sanitary executive—State or municipal—is, as a rule, placing himself in the hands of politicians, who rarely appreciate the importance of the responsibilities resting upon them in dealing with sanitary problems. I think it

is safe to state that the official life of sanitary executives in our larger cities is rarely more than five years; and with but few exceptions, the same is true for similar State officials.

The constructive sanitarians and laboratory workers are in a better position than executive sanitarians, for, as a matter of fact, they are, to a large extent at least, free from political restraint.

It follows from the preceding statements that sanitarians cannot under present conditions keep pace with other scientific workers. The laboratory men may be strong as bacteriologists, chemists, etc.; the engineers may be leaders along their special lines; but there is no executive force to bring all these representative workers together in the solving of sanitary problems. It is time that some well-organized society was taking up this matter. The American Public Health Association is the natural body through which this work should be carried on. It has great responsibilities resting upon it. Its duties were ably outlined by Dr. Wesbrook in his presidential address before the Association at its meeting in Boston last year. The Association should, through Sections, cover all lines of sanitary work. There should be one great body, with its many important branches. A lesson might well be taken from the American Medical Association, which not only has its Sections to cover all important medical work, but is so organized throughout the nation, State and county, as to bring about the best results through the united efforts of its members. Still further, it has its Journal, which reaches all of its members.

Give thought to the great possibilities of the American Public Health Association, if organized along similar lines to those of the American Medical Association. It would then become a national organization, with representatives in every State, county, city and village; a national organization, with its Journal reaching all members, even in the remotest sections of the country; a national organization that could bring to its support all the workers having any relationship whatever to sanitary problems. Such a condition is a possibility

—not a dream. It is simply a question as to whether the American Public Health Association is ready to seize the opportunity to make itself felt as a leader in preventive medicine.

H. M. BRACKEN.

THE RELATIONS OF EXECUTIVE TO TECHNICAL MEN.

The development of laboratories in State and municipal public health work has resulted in the development of a new set of technical men engaged in official public health work, the adjustment of the positions and relations of which to the older established executive health official, has given rise at times to some misunderstanding.

The development of engines and engineers in ocean traffic presents a parallel worth considering. Before the introduction of steam, the captain had in direct charge every detail of the ship's economy—the cargo, the passengers, the sailors, the cook, the navigation; he ordered sails set or taken in as in his judgment seemed best; he and he alone was responsible throughout for everything.

On the introduction of steam, the captain continued to be the executive head, the director of everything, but he could not be absolute in the technicalities of the new department. The engineer is a steam specialist, and the captain, generally, is not. Few men can learn the multifarious and important duties of the captain of a large modern steamer, and at the same time secure the adequate special training of a well-qualified engineer. No man, even if he succeeded in securing adequate training in both lines, could thereafter supply in practice the energy or time under modern high pressure conditions, to properly conduct both, at least in large boats. Hence, perforce, the engineer is independent, but only in his own sphere. He need not know and does not dictate the type of engine, the kind or amount of oil, the particular variety of coal, or the method of firing. The captain is responsible for the sum total results of the voyage achieved; he is fully within his absolute rights in using the engine power to

achieve those results—it is for this that the engines and engineers exist—but the engineer, alone and untrammeled, must develop that power for him, must advise him as to its most economical and efficient use, and may refuse to do what, from his technical standpoint, is impossible or injurious.

In seafaring lines these distinctions between executive and technical are sharply drawn and clear cut. Where both captain and engineer are intelligent, broad-minded and genial, the most harmonious relationships may exist and each may suggest or ask advice without sacrificing for a moment his or the other man's basic rights or independence. Even where these harmonious personal relations do not exist, the official relationships remain clearly defined.

But in public health circles the relationship between executive and technical men, strictly analagous to that of a captain and engineer though they be, are not yet so clearly established. The executive health officer of thirty years ago conducted his health department along established lines. Now specialism has developed in all lines of life and in public health circles just as much as in any other. The technical man has come to furnish the executive health officer with principles of action, and actual power in diagnosis and analysis, such as was undreamed of in the older days. The executive remains and must always remain the responsible officer, the many-sided man of multifarious duties. He must as always direct the application of all the power he can command to this or that object, this or that course. But he can no longer direct the minutiae of the technique of developing or applying that power, even while it is entirely for him to say to what the developed power shall be applied. Of course, as with the captain, he cannot ask impossibilities, and the technical man can often point out how the application of the power to a special object is wasteful or inadequate as well as the converse.

The ideal relations, then, of the executive man to the technical man in public health work must be based first of all upon a due recognition by each of the exact official sphere

and responsibilities of the other. Upon this should be built a broad-minded, intelligent and cordial desire on the part of each to fully understand the other man's problems and difficulties, and to supply his part at the best conditions for the most efficient, most adequate and most economical results. The maximum result from the minimum necessary expenditure of time and energy is the motto of applied science of every description.

The most common fault of which the modern health officer is accused seems to be a desire to use the laboratory for things for which it was not intended, and which it cannot do, while the chief fault of the modern technical man is to consider his work as an end and not as a means to an end.

The modern steamship line does not run in order to give employment to its engines and engineers, but to accomplish certain desired results. The modern public health department is run to reduce morbidity and mortality, not to furnish work or experimentation for the laboratory. But the modern engine and engineer exist because the times call for the most scientific knowledge applied in the most practical ways to achieve the needed results in the most adequate, efficient and economical manner. The modern technical expert exists for exactly the same purposes and reasons.

So in public health affairs, morbidity and mortality will always remain to be attacked and leaders to marshal the hygienic forces against them there must always be. Modern technical work may some day—although now there are no signs of it—be superseded by some more efficient, more direct and more searching method of achieving these ends, but until that day comes the technical man must have proper recognition on his own field of work.

CHICAGO VITAL STATISTICS.

In the Bulletin of the Health Department for January 20, 1906, the Commissioner of Health of Chicago publishes what he says is his last word in answer to those who challenge his

vital statistics. One must regret that the Commissioner will not return to the subject, for the American Journal of Public Hygiene has published some critical remarks on Chicago vital statistics, and it is disappointing that the Commissioner should not have thought some of these observations worth notice. The Commissioner is most fortunate in that seven distinguished physicians have undertaken to say for him as much as he could have said for himself, if not more. The Commissioner's reserve may mean that future inquiry should be addressed to the gentlemen who have relieved him of direct responsibility on this account.

In order to forestall the criticisms and incredulity with which the vital statistics of Chicago have been met for several years past, Dr. Whalen asked the Chicago Medical Society to appoint a "committee of experts" to investigate the local registration office for the purpose of discovering defects and suggesting reforms. The president of the society appointed this committee as follows: Dr. A. C. Cotton, Dr. N. S. Davis, Dr. Adolph Gehrmann, Dr. Ludwig Hektoen, Dr. G. W. Webster, Dr. Weller Van Hook and Dr. J. Allen Patton. The date of this action is not given, but it was later than November 21, 1905. This committee, at its first meeting, appointed a sub-committee "to study in detail the collection, registration and checking of vital statistics." Drs. Gehrmann, Webster and Patton made up this sub-committee. The date of this action is not given, nor the date of the first meeting of the subcommittee, nor is the date recorded of the last meeting of the committee. Their report was unanimously adopted by the Chicago Medical Society on January 9, 1906, and that part of it which is printed in the Bulletin is the Health Department's last word in defense of its Bureau of Vital Statistics. The contents of the report, interesting as they are, do not compare for a moment with the interest of its chronology. This is astounding. It is of most urgent necessity that all the registrars in this country be informed by what means Drs. Gehrmann, Webster and Patton were enabled to complete in six weeks a task which would occupy all the time of

an average registrar and three clerks for six months. The published information concerning the methods employed by the Bureau of Vital Statistics of Chicago might have seemed comparatively unimportant, if the committee had at the same time explained the almost incredible speed of their investigation. The explanation of this unique performance may lie in some simple device whose originator does not know that it is original, or of particular value. By all means let us have the explanation. The secret of such marvellous facility is a thing to be desired above rubies, a cause of importunate yearning, an object of supplication and prayer.

The committee publishes eight reasons for believing "that the Health Department is obtaining reports of and recording all the deaths that occur in the city of Chicago, and is making accurate use of the same in the compilation of Vital Statistics":

"1. No permit for the burial or shipment of a body can be obtained in the absence of a certificate of death properly signed by a physician.

"2. Certificates of 'Unknown Cause' of death are invariably referred to the Coroner for his certificate as to the cause of death.

"3. Bodies found in the lake or waters adjacent to the city, or of persons dying on trains, boats, etc., which are brought into the city are recorded by the Department in the city's mortality, regardless of the place of death.

"4. Deaths occurring at Cook County institutions, even though outside of the city, as the Dunning Asylum, Tuberculosis Hospital and the Infirmary, of all persons that were sent there from Chicago are included in the city's mortality.

"5. Registration of deaths now includes all deceased infants that have breathed, but prior to November 1, 1905, infants surviving less than twenty-four hours were classed as still-births.

"6. It is the committee's opinion that no body can be disposed of in the city of Chicago, by burial, cremation or by shipment in a public conveyance without being recorded by

the Health Department. (a) Bodies used for dissection are receipted for by the Demonstrators' Association and recorded by the Department as having been buried in a certain Medical College, based on a certificate returned by the college clerk. (b) Cemetery clerk's records of burials are checked monthly, and must agree with the Department records, thus preventing surreptitious interments. (c) Railways or other methods of public transportation will not accept a body for shipment without the Department certificate. Shipment of a body surreptitiously by rail or boat would require the connivance of the undertaker, shipping agent, baggage or expressman, receiving agent and undertaker.

"7. The death rate of the city of Chicago is based on the United States Census Bureau enumeration of the population for 1900, with the Census Bureau estimate of annual increase, which is 0.1 of the increase between the census enumerations of 1890 and 1900. This method gives a population for 1905 of 1,990,750, the estimated annual increase being 59,872. These figures in the opinion of the committee give too low an estimate, though probably more accurate than the larger estimates of the School Census or the City Directory.

"8. The records of the Department's investigation of the discrepancies between the United States Census Bureau returns for 1900 and those of the City Health Department clearly demonstrate to the satisfaction of the committee the greater accuracy of the Health Department figures, because of the fact that the Health Department's figures were based on official facts (death certificates), while the Census Bureau's figures were based on inquiries."

The third and fourth of these "reasons" furnish surprising information, which is more briefly stated by the Commissioner, thus: "The Chicago Medical Society states unequivocally and unanimously that not only are all deaths occurring in Chicago recorded by the Bureau of Vital Statistics, but *many others, usually excluded,* are also added to the total upon which Chicago's mortality rate is computed." Registrars have often wondered why Chicago never prints any separate state-

ment of deaths of non-residents, but no one expected to be told that Chicago charges these deaths against the city. Chicago is a great medical centre. Her hospitals include a very considerable population of transients, many of whom die in Chicago. Other cities do not charge such deaths in their mortality accounts, but state them separately, so that everyone may see the difference between the gross mortality actually recorded in the city, and the net mortality on which the death rate is based. Chicago frequently publishes comparative death rates, and the essence of such statements is supposed to lie in the comparability of the data. But it seems that the data of New York, for instance, are not comparable with the data of Chicago; that the Commissioner knows that the figures are not comparable; and that the Committee does not advise the easy correction which would give them at least partial comparability. What is most remarkable, the dissimilarity of mortuary data is unjust to Chicago, causing her death rate to appear higher than it really is; and this amiable prevarication, hitherto carefully guarded from publicity, is only now confessed because some unknown critic has charged Chicago with figuring a low death rate by the vulgar method of inflating her population.

If the Commissioner of Health will not reconsider his resolution of silence in future, many important inquiries about the elements of Chicago's numerical statements will have to remain unanswered. Perhaps the unpublished records of the Medical Society will afford information on certain points not covered by the published report. If the Society is not resolved to be as uncommunicative as the Commissioner, answers to the following queries would be appreciated by the readers of the American Journal of Public Hygiene.

1. Have the published death rates of Chicago in the past five years taken into account precisely the same number of deaths, and the same deaths, accounted for in all the mortuary records of the Department made during the same period?

2. Does the fact that all certificates, alleging "unknown" causes of death, are referred to the coroners, explain the fact

that in the vital statistics for five years, including 130,219 deaths, not one death was charged to "Unknown Causes" (No. 179 of the Classification)?

3. Besides deciding that the Health Department's mortuary records are more reliable than those obtained by the enumerators of the Twelfth Census, did the committee also find that the records added by the Census Bureau to the mortality registered by the city represented deaths which did not occur in Chicago?

4. In the Bulletin of July 22, 1905, the Commissioner says that the death rate of Chicago in 1873 was 27.6, and the average age at death 15.3 years, while in 1904 the death rate was 13.6, and the average age at death 32.5 years. From these figures the Commissioner derives the conclusions that "In a single generation the death rate of Chicago has been reduced one-half, and the duration of life within its boundaries has increased twofold." Does the committee believe that the figures warrant the Commissioner's conclusions?

5. In the Bulletin of January 27, 1906, the Commissioner says that the average age at death in 1869 was 13.92 years, and in 1905 the average age at death was 31.83 years, and that these figures demonstrate "an increase of 138.7 per cent. in the average duration of life in this city." In the same Bulletin the Commissioner prints a table showing that the average age at death in 1875 was 16.17 years, and in 1905, 31.83 years. Over this statement he prints a small cap head, as follows: "Increasing Duration of Life in Chicago Doubled in a Single Generation." While approving the past work of the Bureau of Vital Statistics, did the committee make any reservation, or offer any admonition, concerning the future publication of such atrocious nonsense?

6. Did the committee verify the Department's statement, published in 1904, that pneumonia caused 22.32 per cent. of the total mortality, and that 21.8 per cent. of all deaths occurred in the age period under five years? If so, did they note the phenomenal disturbance that must have resulted

either in the age distribution of pneumonia, or in the seasonal distribution of infant mortality?

7. Did the committee verify the statement of the Department in 1904 that measles declined to 9-190 and whooping cough to 4-93 of its former mortality, while pneumonia rose at the same time from 16 per cent. to more than 22 per cent. of the total mortality? If so did they find any explanation of this unique and most fascinating statistical topsy-turviness?

The basis of prevailing skepticism concerning the vital statistics of Chicago consists in the illusions which the figures create in the minds of the official statisticians. It is not necessary to suppose that the statisticians deliberately succumb to these illusions. However sincere they may be, and however true the figures themselves may be, the fact that fantastic misinterpretations of the statistics are frequently published, justifies a doubt whether men who are so easily misled by figures can keep numerals in order at any stage of their work.

JOHN S. FULTON.

Baltimore, January 30, 1906.

ANNUAL MEETING

OF THE

Massachusetts Association of Boards of Health.

The annual meeting of the Massachusetts Association of Boards of Health was held at the Hotel Brunswick, Boylston street, Boston, on Wednesday, January 24, 1906. Vice-President Chapin presided.

VICE-PRESIDENT CHAPIN: Ladies and Gentlemen, if I had had any idea last year when I was elected Second Vice-President that I should be called upon to do any work, I should not have ventured to accept the honor. Unfortunately for the Association, both Dr. Durgin and Dr. Walcott are absent today, so I shall have to ask you to bear with me. The Secretary, Mr. Coffey, also is absent, so I suppose that the first business of the meeting will be to elect a Secretary pro tem.

(On motion of Dr. Field, Mr. B. R. Rickards was elected Secretary pro tem.)

VICE-PRESIDENT CHAPIN: Has the Executive Committee any business to report?

DR. FIELD: The Executive Committee nominates Dr. George B. Magrath, assistant to the Secretary of the State Board of Health, for membership.

VICE-PRESIDENT CHAPIN: You hear the report of the Executive Committee. They nominate Dr. George B. Magrath as a member of the Association. What is your pleasure?

(On motion of Dr. Palmer, and by vote of the Association, the Secretary pro tem. cast one vote for Dr. Magrath, thus electing him to membership.)

VICE-PRESIDENT CHAPIN: The next order of business is the report of the Treasurer.

DR. FIELD: Annual report for 1905.

REPORT OF THE TREASURER.

Receipts.

Balance from 1904	$1,100.59
Interest	37.21
Annual assessments	531.00
Total	$1,668.80

Expenses.

Postage	$56.60
Printing	28.85
Cigars and dinners for guests	30.45
Clerical assistance	9.75
Treasurer's bond	4.00
Report of meetings	100.55
Printing Journal, two numbers	109.13
Express	.40
Total	$339.73
Balance to 1906	1,329.07
Total	$1,668.80

Of this balance, $1,020.78 draws interest in the Central Savings Bank, Lowell.

Respectfully submitted,

JAMES B. FIELD, Treasurer.

Examined and approved as correctly cast and properly vouched for. J. ARTHUR GAGE, Auditor.

January 18, 1906.

In explanation of our large surplus, $1,329, I should say, as I have said in many years, that the prosperity of the Association depends upon the prosperity of the Journal. When the Journal is paying for itself we can accumulate a surplus.

I hope that surplus will eventually get so large that our dues will only be $1 per member. But when we make a new contract for the Journal, and cannot make a good contract, it would only take the issue of about three or four numbers of the Journal to wipe the entire surplus out and put us into bankruptcy. So it is essential that we should have some surplus stored away to come and go on.

VICE-PRESIDENT CHAPIN: You hear the report of the Treasurer. What action will you take?

MR. UNDERWOOD: I move that it be accepted, and placed on file.

(The motion was seconded and adopted.)

VICE-PRESIDENT CHAPIN: The next item on the order of business is the report of the Secretary.

(Mr. Rickards, Secretary pro tem., read the records of the October, 1905, meeting of the Association.)

VICE-PRESIDENT CHAPIN: You hear the report of the Secretary. If there is no objection, it will be accepted.

The next business is the election of officers. What is your will in regard to the election of officers?

DR. HORACE E. MARION: I move that the nomination of officers for the ensuing year be made from the floor.

(The motion was seconded and adopted unanimously.)

VICE-PRESIDENT CHAPIN: Nominations for officers for the ensuing year are in order.

DR. HORACE E. MARION: Mr. President, I need not go into the history of the Association, or the good work that

it has done. If I am not mistaken, our very efficient Secretary conceived the idea of having the Boards of Health of the various towns and cities joined in an association like this, and he consulted the present officers of the Association and formed this organization. Whatever of distinction the Association has made outside of the State, and whatever it has accomplished within the State, is certainly due to their most efficient and gratuitous service. At the last annual meeting the nominating committee volunteered the suggestion that the tenure of office be limited. Such a recommendation, it seems to me, was entirely out of order. It was not put before the Association at that time. Such a measure should be brought up as miscellaneous business, and discussed in the Association, and acted on. It was not so discussed, and it has not been mentioned since then. It thus puts the Association and its officers in a very unpleasant light,—the Association, because we are simply responsible for a deed that is manifestly not the wish of the Association; the officers, because they certainly would not serve if there was any dissentient feeling. You know, gentlemen, how you stand in the Association upon such a movement as this. You know, too, how much is due to the officers of our Association for what we have succeeded in accomplishing in the past. A hint such as was made by the suggestion is sufficient to queer the present officers. I hope the action of this meeting will be such as to insure the retention and re-election of the past officers of the Association. (Applause.)

MR. CARY: I heartily second the member's motion. I think our officers have brought the Association up to the standing it has today, and have done it where perhaps no other men in the State could do it. Situated where they were, with the influence that they had throughout the State and throughout the country, I think they have brought the Association up to date where it would not have been if it had not been for them. I certainly hope the Association will elect those officers by a unanimous vote.

DR. NEWCOMB: Mr. Chairman, if in order I would like to nominate Dr. Henry B. Walcott, Dr. Samuel H. Durgin, Dr. Charles V. Chapin, Dr. James B. Field and Mr. James C. Coffey to continue in the positions which they have so long and so ably filled. That is my motion.

DR. PALMER: Mr. Chairman, I have been waiting in hopes the chairman of the committee that made that suggestion, Prof. Sedgwick, would speak for the committee. I find that during the year there has been a misunderstanding as to what the recommendation of that committee a year ago was. I was the third member of that committee, and the suggestion that was made was, that in the lesser positions, it might be for the advantage of the society that a change of men be made, that different parts of the State might be represented. It had no intention of changing the heads of the Association. As I remember it, the chairman of that committee, who brought in the report, fully explained that point. Inasmuch as he is not here, I want to explain that the committee had no thought of lack of appreciation of the faithful work that has been done by these first officers, or wanted them changed. I give this simply in explanation and in correction of what I think has been a misapprehension on the part of the society as to the work of that committee at that time. I gladly second the motion.

VICE-PRESIDENT CHAPIN: Are there any other nominations? If not, I will ask the Secretary to put the question of the election of the officers as nominated.

(The motion was put by Mr. Rickards, Secretary pro tem., and the five gentlemen nominated were unanimously re-elected.)

VICE-PRESIDENT CHAPIN: It is now necessary to elect five new members of the Executive Committee, to serve for two years. What action will you take in regard to the election?

DR. WORCESTER: Mr. President, will you please read the names of the present Executive Committee?

VICE-PRESIDENT CHAPIN: G. E. Tobey, W. H. Gove, G. H. Ellis, C. V. Chapin, H. C. Emerson, F. W. Kennedy, W. C. Kite. Of course, C. V. Chapin really should be dropped from that committee, as he is Second Vice-President and *ex officio* a member of the committee. The gentlemen whose names I have just read are the ones whose term of office expires now. There are five other members, who were elected last year for two years. There are five men to be elected, then, at the present time, as members of the Executive Committee for the next two years. I shall be glad to hear nominations.

DR. NEWCOMB: Mr. Chairman, if in order I move that we appoint a nominating committee.

(The motion was seconded.)

VICE-PRESIDENT CHAPIN: It is moved and seconded that a nominating committee be appointed to suggest names for the Executive Committee for the ensuing two years.

DR. WORCESTER: I move, as an amendment, that the committee consist of three members and be appointed by the Chair.

(The motion, as thus amended, was adopted.)

VICE-PRESIDENT CHAPIN: I will appoint Dr. Worcester, Dr. Harrington and Dr. Smith.

DR. TOBEY: I would like to say to this committee that I hope they will have no hesitation in dropping my name from the Executive Committee. I have been a member of the Executive Committee continuously since the organization

of this Association, and I think it is about time that I should give way to someone else.

VICE-PRESIDENT CHAPIN: Is there any other business to be brought before the Association?

A gentleman who sits beside me, who is connected with the Boston Board of Health, reminds me that it is customary at this meeting for the Chairman to suggest that invitations from Boards of Health for the April meeting are in order and would be gratefully received.

The nominating committee is ready to report.

DR. WORCESTER: Mr. President, Members of the Association:—Your committee wish to submit the following names. We found that there were five men to be nominated for two years, and that there were two men who, according to our by-laws, should be nominated, that is, two more, to fill out one year's service. There were not enough members on the Executive Committee. For two years we would nominate Dr. Walcott P. Bowers, of Clinton; Dr. L. M. Palmer, of Framingham; Dr. E. H. Stevens, of Cambridge; W. L. Underwood, of Belmont; Dr. G. Arthur Bodwell, of Salem. For one year, Dr. Wesley T. Lee, of Somerville, and Dr. J. T. Bullard, of New Bedford. That is the entire list.

VICE-PRESIDENT CHAPIN: Gentlemen, you hear the nominations brought in by the committee. What is your wish?

DR. HARGRAVES: I move that the report be accepted.

(The motion was seconded and adopted, and the gentlemen named were then elected members of the Executive Committee by unanimous vote.)

VICE-PRESIDENT CHAPIN: If there is no other business to be brought before the Association, we will proceed to

the reading of the papers. Of course, there is no necessity for introducing the first speaker. We are all delighted to hear him. We are not only delighted to hear him, but we always go away with a decided increment to our scientific knowledge. It gives me great pleasure to present Professor Theobald Smith, who will read a paper upon "What Is the Relation Between Human and Bovine Tuberculosis, and How Does It Affect the Inmates of Public Institutions?"

WHAT IS THE RELATION BETWEEN HUMAN AND BOVINE TUBERCULOSIS, AND HOW DOES IT AFFECT INMATES OF PUBLIC INSTITUTIONS?

Dr. Theobald Smith, Harvard Medical School, Boston, Mass.

The progress made in the past twenty-five years in the study of infectious diseases, notably those of the higher domestic animals, has placed us in a somewhat different position with regard to the question of the intercommunicability of infections between animals and man. When the tubercle bacillus was discovered there was but little known of the definite limitations of microörganisms to certain hosts. Then pathogenic power toward one of the higher animals meant rather vaguely danger to all others. Now pathogenic power of any microbe toward a given animal species means that and no more until more has been proved. We may protect ourselves in actual life against possible dangers, but we have not thereby proved that these dangers are real. Unfortunately much of the anxiety we feel toward infections emanating from the lower animals cannot be placed at rest because the experimental method, the actual test, is inapplicable, and we must content ourselves with roundabout methods, clinical observations, inferences from analogy and painstaking comparisons between the microörganisms if they can be seen and cultivated.

To what extent infectious agents are restricted in their

activities is shown by the fact that only one of the infectious diseases of man—the bubonic plague—is shared by one of the lower animals, the rat. The plague is known to spread freely from rat to rat, from rat to man and less freely from man to man, so that some are inclined to the view that the plague is primarily a rat disease. There are other infections which may be transmitted from animals to man, but only under exceptional circumstances. Man does not, as a rule, transmit these to his fellow-man, chiefly because the necessary machinery for transmission is absent.

After the publication of the celebrated work of Koch in 1882, it was generally conceded that bovine and human tuberculosis were absolutely identical, and that ingestion or inhalation of the bacilli from one source was as dangerous as from the other. Facing this problem of intercommunicability from the standpoint of today, the burden of proof really rests upon those who admit such a free transmission from man to animals and the reverse. For we cannot but assume that two bacilli of highly parasitic character, one living in cattle and the other in man, must eventually develop irreconcilable differences stamped upon them by their respective hosts, unless such remarkable mutual affinities exist as those manifested by the plague bacillus. But these should be demonstrated rather than assumed.

It was reasoning of this kind coupled, with experimental observations that led me in 1894 to study more in detail cultures of tubercle bacilli from different hosts. This is a tedious process. It was not until 1898 that a series had been studied sufficiently large to justify the establishment of two races or types of tubercle bacilli, a bovine and a human type, although I had demonstrated in 1896 profound differences between the two types. This investigation led me to take the position that it was impossible to tell precisely how such differences in the bacilli influenced intercommunicability, and that we must now begin to study cases of human tuberculosis to determine more accurately which types of disease may be referred to animal sources.

At the same time I contended that these results contradicted the extreme fear with which bovine tuberculosis was being regarded, and that it did not justify the very heavy expenditure of public funds in the destruction of all tuberculin-reacting animals, and what was still worse—the condemnation by law of the flesh of even the mildest, most insignificant cases as unfit for human food. This position was all the more necessary, since the importance of the strictly human source of tuberculosis, the sputum, was being pushed to the background, and our State was appropriating not less than a quarter of a million dollars a year for the eradication of bovine tuberculosis.

Now it is possible that we may be threatened again by legislative schemes to pay out of the public treasury money to defray the immunization of young cattle according to von Behring's scheme. What the State can justly do is to pay for suitable experiments to study this method and to prepare the vaccine, but nothing more. If the State has any money to spare, it should be reserved for the care and housing of human consumptives.

The work of studying the tubercle bacillus along lines laid down by me was being carried on quietly, notably by Ravenel in Philadelphia. It was not, however, until 1901, when the great authority of Robert Koch placed itself directly across the path marked out by sanitarians since the publication of his great discovery in 1882. He maintained not only the radical distinction between bovine and human tuberculosis, but went so far as to regard the bovine disease as a negligible quantity in our struggle against the human disease. While he may be said to have gone too far in this latter position, his views exercised a salutary influence in forcibly drawing the attention of the world's sanitary machinery *from a minor to a major* source of disease, and without doubt the public anti-tuberculosis movements of today, of which this meeting is a part, gained an enormous impetus through his somewhat one-sided position.

The wave of opposition raised by Koch's address led to the

appointment of a number of commissions to investigate the subject anew. The English Government had only a few years before issued a very large report by a royal commission in which the bovine and the human disease were regarded as identical. A new commission was formed at once, which established perhaps the most elaborate temporary plant in the world for the re-investigation of this question. A leaflet issued several years ago leads us to infer that in its final report the commission will, adhere to the platform of the former commission. A royal commission appointed by the imperial health office of Germany has recently issued a series of reports in which it places itself wholly on the ground taken by me in 1898.

Many minor investigations upon this subject have been made and published during the past four years which range themselves on one side or the other of this sanitary problem.

If we briefly summarize the results of these investigations we find two facts firmly established: 1. The conception of two types of mammalian tubercle bacilli, a bovine and a human type. 2. The occasional presence of the bovine type in cases of human tuberculosis.

How often the invasion of the human subject with bovine bacilli takes place cannot be stated with any degree of accuracy for all investigators have turned their attention chiefly to cases originating in the digestive tract or its associated lymph nodes, *i. e.*, to those cases in which infection through raw milk may have taken place. We are, however, in the position to affirm that the old notion that all tuberculosis originating in the intestinal tract is of bovine origin is not true. Of five such cases studied by me, one was associated with bovine bacilli. Of twelve studied by the German commission, six were associated with bovine and one with both kinds of bacilli. Ravenel had reported several cases, similarly the Department of Agriculture at Washington. I have not at hand the means for calculating the percentage of intestinal tuberculosis associated with bovine bacilli, but it may be roughly estimated at twenty to forty per cent. This

may seem a relatively high rate, but the following circumstances must be taken into consideration:

1. A certain proportion of these cases are stationary. The disease is restricted to the mesenteric lymph glands.

2. Where tubercular ulcers due to the human bacillus exist, the bovine bacillus in the milk may be admitted, when this might be impossible with an intact mucosa.

3. Tuberculosis of intestinal origin is a relatively rare affection.

4. In Japan, where milk does not play any appreciable role in the feeding of infants and children, human tuberculosis, including the intestinal disease, is as common as in other countries.

I can merely refer, in passing, to the question of the transformation of the bovine into the human bacillus in the human body. If this were possible, our efforts to trace the sources of tubercular infection by bacteriological methods would be fruitless. While I am prepared to admit a certain change manifested by reduction in virulence the evidence we possess is conflicting and hardly in favor of the transformation theory.

It is needless to state that much more work must be done before we can formulate precisely the relation which the bovine bacillus bears to tuberculosis of alimentary origin. In the meantime, certain broad facts stand out above the fog of controversy to which those to whom the health and well being of large numbers are entrusted may cling with a reasonable amount of confidence.

In the first place, the bovine bacillus has been found almost exclusively in children under ten years of age. Phthisis and the other forms of tuberculosis of adults are, so far as we know, almost wholly of human origin. Institutions in which children are cared for, the source of the milk should receive careful attention and herds belonging to the institution should be free from any traces of this disease. In institutions harboring adults there need be less anxiety on this point, and if all animals of reduced vitality or whose udders are not above suspicion are eliminated, the danger of infec-

tion may be regarded as exceedingly small. This standpoint is based upon the following well-established facts:

1. With few exceptions cattle reacting to tuberculin are in an early or latent stage of the disease during which tubercle bacilli are not excreted in the milk.

2. When tuberculosis becomes generalized, which frequently happens after parturition, and the udder and uterus become affected physical examination and microscopic examination of the secretions should detect it.

To these we may add that it is highly probable that we can successfully resist an occasional tubercle bacillus and that most of us, at one time or another, have ingested without harm a few bacilli in milk or butter. It is equally probable that our resistance may give way at an unguarded moment when our alimentary tract is being daily flooded with bacilli from a tubercular udder.

In view of these data it is obvious that a careful periodical inspection of dairy herds cannot be dispensed with and that such inspection should be made a necessary function of State and local boards of health or of specially organized bureaux in order that any advanced cases of tuberculosis may be detected, removed and destroyed. More than this cannot be demanded at present in the interest of public health. Yet it should be the pride of the head of every one of our public institutions to keep the dairy herd free from tuberculosis. How this can be done Dr. Austin Peters, who follows me, will be able to tell us.

VICE-PRESIDENT CHAPIN: I know you have all been as interested as I have been in what Dr. Smith has told us, and it is very refreshing to have the bacteriologists take such a sane position as he has taken and explain their views as clearly as he has. I would call upon Dr. Peters to open the discussion of this paper.

DR. PETERS: Mr. Chairman and Gentlemen, I have been asked to follow Dr. Smith with a few words on bovine

tuberculosis, I suppose, perhaps, more with reference to the existence of bovine tuberculosis in herds of public institutions. Dr. Smith has already told us that until within a few years the tubercle bacilli found in cattle and other animals were supposed to be identical with those found in humans; but more recent researches have shown that there is a difference between the bacillus found in human sputum and the bacillus found in lesions in cattle, and a large amount of the credit for ascertaining this difference at first is due to Dr. Smith's studies. I think Koch has observed the same differences, but I think Dr. Smith's researches led to Koch's noticing the difference later. Koch was the original discoverer of the tubercle bacillus, and at first thought that it was identical in animals and man. Originally the tubercle bacillus may have been the same organism, but as a result of its growth for many generations in different hosts it has assumed changes, so that there are varieties, perhaps, of the same organism, but the changes have become so fixed that very likely one form will never revert to that of the other. The human tubercle bacillus has certain differences from the bovine bacillus that characterize it. Probably it would take a great many generations of cultivation under certain conditions to make one identical with the other again, and perhaps it would be an impossibility to do this.

The sputum bacillus is virulent to mankind, but it seems to have very little virulence toward cattle and the lower animals. On the contrary, the bovine bacillus is very virulent in comparison to the sputum bacillus to cattle and to other lower animals and small experimental animals. As the human tubercle bacillus is of very little danger, evidently, to cattle and other animals, and as the bovine bacillus is very virulent to them, it is assumed that the converse is true, and that there is very little danger from the bovine bacillus now to mankind. At the same time, as Dr. Smith has told you, there are instances where the bovine bacillus is found in lesions in children. The truth of this seems to be borne out by the facts, because it is so difficult to trace a case of tuber-

culosis in a human being to cow's milk. We know that if a man has glanders, it is a very easy matter to find out the horse he got it from; if a man has rabies it is not a difficult matter to find out the dog with rabies that bit him, but if a person has tuberculosis it is often very difficult to trace the source of infection. I think one reason for this is, as Dr. Smith has said, that the infection may be taken into the body and remain latent for a long time in some lymphatic gland, and then under suitable conditions develop. It is very hard to trace a case, so as to say that it came from cattle, without eliminating other possible sources of infection. Then we know that phthisis is no more common in persons working in cow barns than in people following other pursuits, showing that certainly there is very little danger from pulmonary infection when working among diseased cattle.

The experiments tried a number of years ago, under the auspices of the old Society for Promoting Agriculture by Dr. Ernst and myself showed that tuberculous cows, even without having tuberculosis of the udder, could give bacilli in the milk. The cow with a bad case of tuberculosis, even with a healthy udder, can excrete bacilli in the milk. If a cow has a tuberculous udder, of course she is a greater source of danger, and milk from cows of either kind fed in an uncooked state to calves or pigs will produce tuberculosis in them, and if it is injected hypodermically into rabbits or guinea pigs, tuberculosis can be produced in them experimentally. I happen to think of a case where a lot of pigs that were kept at a creamery in New Hampshire, I think there were 100 pigs, were sent down to one of the pork-packing establishments near Boston to be killed. They were fed on the skim milk and buttermilk from the creamery. Of the 100 pigs, 19, or nearly 20 per cent., were condemned by the inspector of the United States Bureau of Animal Industry as tuberculous and unfit for human food. This was probably a milk infection. No one wants to advocate the use of such milk for invalids and children, and the importance of a healthful milk supply should not be overlooked in the consideration of tuberculo-

sis, even if the danger is less than was supposed at one time.

Dr. Smith spoke of the various suppositions as to the infection of human beings with tuberculosis. One was that it was acquired in utero and then developed later in life. This in cattle is an unusual occurrence, and I do not believe that it is any more frequent in human beings. The calf is nearly always born healthy. We know that it is a perfectly possible thing to raise a healthy herd of cattle from a diseased herd. We can take cows with tuberculosis and a tuberculous bull, and keep them in one barn, and take the new-born calves as fast as they come and put them in another barn, where the disease does not exist, and bring them up on healthy cows' milk, and test them at the end of the year with tuberculin, and not more than 2 or 3 per cent. will react. This shows that the infection, as a rule, comes after birth. It seems to be the will of a wise Providence that, as far as tuberculosis goes, the offspring shall, in nearly all cases, have a fair show in the world, and start healthy, and the disease is, in the majority of instances, acquired after birth; while congenital tuberculosis does occur it is very infrequent.

Even if we admit for the sake of argument that there is no danger from the use of milk from tuberculous cows, or the meat products from tuberculous animals, there is another aspect which still remains, and that is the commercial one. Walley classifies tuberculosis among cattle with the four great bovine scourges, the other three being rinderpest, foot and mouth disease, and contagious pleuro-pneumonia. Certainly, a disease which causes the losses to cattle owners that this does is one that no farmer and no public institution can afford to have in a herd if it can be eliminated, because it will cause annual losses, the cattle will not do as well, occasionally one will die with it, and I do not think cows give as much milk or thrive where the disease exists in a herd as where the herd is healthy.

As far as tuberculosis in public institution herds goes, I think a public institution in the State of Massachusetts that

has not had experience with tuberculosis in its herd is the exception and not the rule. The one obstacle to the freeing of public institution herds from tuberculosis seems to be a lack of fixity of purpose in the management. Every institution has a board of trustees and a superintendent, and occasionally one will get a new superintendent or some new member of the board of trustees, who will find that the herd is tuberculous and start to eradicate tuberculosis from it. He will have the herd tested and the reacting animals killed, and will disinfect the barns, and only put in tested animals for a while. Then the time of that member of the board of trustees expires, and a new man is appointed, or a new superintendent comes, and they are careless about buying cows, and will buy an untested cow that has tuberculosis, and stop testing the herd once or twice a year, and the first thing you know the herd is as bad as it was before any attempt was made to clean it up. Where this work has been done by the State, and at the State's expense, in a great many instances the money has simply been the same as thrown away. I could give instances of that kind, if necessary. The sporadic effort is of no avail, and it is only a definite and prolonged purpose that will give results that will be productive of good. A herd can be freed by means of the tuberculin test if the stable is thoroughly disinfected, and only tested cows are bought in the future, and the herd is tested once or twice a year. Any intelligent farmer can learn to do this without the State's assistance, but I do not think very many do it.

Recently experiments have been tried in immunizing cattle to protect them from tuberculosis. Koch has done some work in this line, and von Behring. Von Berhing has produced what he calls "bovovaccine," probably an attenuated culture of a human sputum bacillus, a small quantity of which is injected into the jugular vein of a calf two or three weeks old, and then in three months the calf receives another intravenous injection of the same material, five times the size of the first dose, and this is said to produce immunity in the calves. Perhaps if older cows could be immunized in the

same way before being put into herds where tuberculosis exists it would be a good thing.

There is a chance for a good deal of study along this line yet. All the cows brought into Massachusetts from without the State are tested either before shipment or after arrival, and most of them after arrival. Last year there were about 20,000 cows brought into Massachusetts, and 13,000 were tested at the stock barns at Brighton by agents of the Cattle Bureau, and of the other 7000 over 4000 were tested after arrival by agents of the Cattle Bureau. Only about 2800 came in on certificates of tests. So we are getting in a position where we are surer that the testing is more honestly done than formerly, and if farmers would only take pains to buy tested cows in making new purchases, and would disinfect the places where diseased cows stood whenever they disposed of any, a much better condition of affairs would be brought about than exists at present.

I do not think, considering the amount of money that the State has expended in assisting farmers to rid their herds of this disease and protect the public health, that the farmers have co-operated with the State authorities as fully as they might; in fact, a great many of them have opposed the State. They have been opposed to the tuberculin test, and called tuberculin a humbug, and an injury to the cow, and a poison, and have done all they could to hamper and impede the work of the Cattle Bureau rather than to assist it. As far as the Cattle Bureau's work goes, at the present time it seems to give the State of Massachusetts a fair system of dairy inspection. We take 1500 or 1600 bad cases of tuberculosis a year from the dairy herds in Massachusetts, and these are cattle that can be easily condemned on a physical examination or have diseased udders. The last year the State Board of Health has also adopted a system of dairy inspection, and has a veterinarian examine the dairy herds. Where he finds a case of tuberculosis it is reported to the Cattle Bureau and an agent is sent to see it, and the cow is appraised and killed. This inspection is perhaps of more benefit to people in Massa-

chusetts from herds owned in the State, but it does not extend the same benefit to consumers of milk coming from outside of the State, and as a great deal of our milk comes from New Hampshire and other States, the same good is not done beyond our boundaries, particularly by the Cattle Bureau.

There is not time here to say much about sanitation in our cattle barns, further than that they ought to be light, well-drained, well-ventilated and clean. With the tuberculin test, better sanitation, and possibly a practical method of immunization, I think that very much could be done towards freeing our herds from tuberculosis; but even without a practical method of immunization, with tuberculin alone, we know that we can free a herd from tuberculosis, and if the owner will buy only tested cows, and test the herd twice a year, spring and fall, he can keep a clean herd. Sanitation alone is not sufficient. It may work very well with people, but I doubt if sending tuberculous cows to the State sanitarium at Rutland would avail very much for them. I think that if the tuberculous cows that have showed evidence of disease are destroyed and those that react to tuberculin are taken out of the herd, with that sanitation will be a help, but sanitation alone will not do everything for the cattle, as much as it will for people.

I think, gentlemen, this is all I have to say about the dairy cow.

VICE-PRESIDENT CHAPIN: The discussion of Dr. Smith's paper is now open to the house.

DR. GAGE: I would like to ask Dr. Smith one question, for information, as to whether if a farmer imports cows he is obliged to have them inspected by the local inspector in the district in which he lives, or whether he can select any inspector endorsed by the State Board.

DR. PETERS: Any one who brings into Massachusetts cattle upon which a test is required is entitled to have his animals tested by an agent of the Cattle Bureau. If cattle

are brought in from without the State I send some one to test them.

DR. MILLER: I would like to ask the doctor if all cattle brought into the State are tested.

DR. PETERS: No, I think there are some cattle that are brought across the line from New Hampshire and Vermont by people in abutting towns, who bring them over in the night without saying anything, that are not tested. I think all the cattle brought in by rail are tested, because we hold all the cattle that are brought to Brighton and test them there, that is, all the cattle except beeves, for immediate slaughter and calves under six months old. At points outside they have to have a permit to bring the cattle into the State, and the railroad won't take them without a permit. But where a man can drive cattle from New Hampshire, Vermont or Connecticut along the road into Massachusetts, I think some are brought in in that way without our knowing anything about it.

VICE-PRESIDENT CHAPIN: As you see, Dr. Durgin had arranged that I should read the next paper. I am sorry to take up any more of your time this afternoon. I will try to be as brief as possile. What I wanted to do was to discuss the causes of death in Providence. I have prepared a table which shows the deaths per 100,000 of the population from all the different causes by five-year periods for fifty years. That, of course, was too large to bring here, but I have brought a table in which the first-column figures show the average number of deaths per 100,000 for the whole period of fifty years. Thus, for fifty years, we have averaged 45 deaths per 100,000 from typhoid fever. In the beginning of that period, the first ten years, the deaths were 64; the last they were 17. They increased somewhat, and then fell off again. The difference between 64 and 17 was 47, so that during this period the disease diminished 47 per 100,000. That is the meaning of the two sets of figures.

CAUSES OF DEATHS IN PROVIDENCE, 1856-1905.

By Charles V. Chapin, M. D.

Superintendent of Health, Providence, R. I.

During the period under consideration the crude death rate in Providence has fallen from 20.87 to 18.90 per thousand. As you doubtless well know, a crude death rate is of very little value as an indication of the sanitary condition of a city. Mr. Hoffman, actuary of the Prudential Insurance Company, read an interesting paper last week before the American Statistical Association in which he made use of over twenty factors in correcting crude death rates, and stated that as many more might be employed. Among important factors influencing the death rate are age, sex, race and social condition. Probably the most important factor influencing the death rate is the age distribution of the population. Many of the other factors considered by Mr. Hoffman are more or less dependent upon this.

One of the important uses to which vital statistics are put is to enable health officers to judge whether or not their efforts have been successful, and to suggest direction for their future activity. For this purpose we must study the different causes of death. Among the diseases to which health officers have given the most attention are typhoid fever, scarlet fever, small pox and diphtheria and croup. A glance at the table shows that all these diseases have diminished and very markedly so. The decrease, however, is partly due to a decided decrease in the population at those ages most subject to these diseases. The apparent decrease in these four diseases amounts to 214 per 100,000, of which perhaps a quarter is due to different age distribution. I am of the opinion that a large part of this decrease is largely due to the conscious effort of the municipality. The decrease in typhoid fever is probably due almost entirely to the introduction of a public water supply, its improvement, the abolition of privy vaults, etc. The decrease in small pox, scarlet fever, and diphtheria

have doubtless been largely due to measures of isolation insisted on by the health department, to vaccination, and the distribution of antitoxin.

Two other diseases show a great apparent decrease, and a similar decrease in other cities is usually claimed by their sanitary officials as due to their own efforts. The decrease in diarrhoeal diseases is apparently 79, but it is very difficult to determine what the real decrease has been. Most of the deaths from this class of diseases are at ages under five years, and the proportion of the population in Providence, at this age, has decreased about 25 per cent., a greater decrease than the diseases have shown. On the other hand, improved methods of diagnosis have probably transferred some deaths from meningitis, marasmas and debility to diarrhoea. On the whole, I judge there has not been a great deal of decrease in this group of diseases, and this is further indicated by the fact that the ratio of deaths under one year to births, has shown an increase of 34. While the health department of Providence has not accomplished much in the prevention of these diseases, yet more can be accomplished if there can be exercised proper methods of controlling the milk supply, and means devised for instructing mothers in the care of their children. The whole subject of infant mortality is one that needs more careful study by health officials, and I know of no more promising field for their efforts. The disease which has shown the greatest apparent decrease during the last fifty years is consumption. There were 169 less deaths per 100,000 during the last ten years. The reality of this decrease has been questioned in many quarters, but I am inclined to think that it is very largely a real decrease. It is indeed probable that some cases may have been transferred to pneumonia for the purpose of defrauding insurance companies, but I am satisfied that in Providence this number is not large. On the other hand, it is likely that in former years many cases of consumption were incorrectly diagnosed as bronchitis. Changes in the age distribution of the population would tend towards increase to a considerable degree. Race

is an important factor in this disease. Hebrews are much less susceptible to it than other white races and their recent influx to cities like Providence and Boston would tend to its decrease. But in Providence there has been a marked decrease in consumption among persons of American parentage, which class contains an increasing number of Irish descent who are especially susceptible, so that there seems to be no doubt that consumption has decreased decidedly independent of the age or race distribution of the population. Little credit for this decrease in consumption is assumed by the health department in Providence. It is possible that the efforts of the department, and of practising physicians have accomplished a little in preventing infection in this disease, but it is probably very little. In this connection it may be noted that tuberculous diseases other than consumption have shown a quite decided apparent increase from 0 to 40 per 100,000. It is probable that this is wholly due to improved diagnosis. Thus it can be seen that scrofula has diminished 20, or half the above amount, and has probably in reality been transferred to tuberculosis. Other cases of tuberculosis were formerly returned as marasmas, meningitis, or enteritis, or hydrocephalus.

Your attention is now called to several other diseases which are usually classed as preventable, and which health officials attempt more or less vigorously to restrict. These are measles, whooping cough, cerebro-spinal-meningitis, influenza, and pneumonia. According to the chart, measles has increased five, and whooping cough has diminished eight. If the two were taken together it will be seen that there has been little apparent change in their mortality, and this I think is correct. When it is remembered that the population at the susceptible ages has decreased, no credit is due the health department for controlling these diseases. Very little has been done towards controlling whooping cough in Providence, and only half-way measures have been adopted in regard to measles. Probably in both these diseases it makes no difference whether the most stringent measures are adopt-

ed or nothing is done. The health officer of Aberdeen has recently issued a most interesting report on measles. For over twenty years that city attempted to control measles as we in this country attempt to control scarlet fever. The disease remained practically stationary, and the mortality did not differ much from that in other Scotch cities where practically no restriction was attempted. It is worthy of note that at the present time the mortality from each of these diseases is greater than that of scarlet fever. As I have tried to show on other occasions, restrictive measures in contagious diseases will not be successful unless a large proportion of the infected persons can be controlled from about the beginning to the end of the period of infection. This we cannot hope to do in these two diseases, and hence the failure of all attempts. For similar reasons health officials have been able to do nothing to check the spread of cerebro-spinal meningitis. This disease in Providence has shown an increase from 0 to 8. Owing to the large number of well persons who are infected with the influenza bacillus and the pneumococus, restriction can avail nothing in the diseases caused by them. Influenza appeared in Providence in December, 1889, and caused the maximum number of deaths during the next five years, since which time it has diminished. Pneumonia has shown an apparent increase of 51. It is to be doubted, however, whether this is real. During the last fifteen years it is probable that many cases of influenza have been reported as pneumonia, as suggested by the culmination of the pneumonia mortality in the same period as that of maximum influenza mortality. Allowing for the increase due to improved diagnosis and to change in the age distribution of the population, it is doubtful if pneumococcus pneumonia has shown any real increase.

It is really surprising how few diseases there are among those common in New England over which health officials can exert any real control. The common belief is that the health of the community rests almost entirely with the board of health, and if there is any marked decrease in the general

death rate credit for it is almost invariably claimed by the officials in charge. It may be objected that the municipality has done much to reduce the general death rate over and above that caused by the contagious diseases by improving the sanitary conditions so called. If by this is meant municipal cleanliness, no one has ever shown how it has any effect upon the death rate except in a limited number of diseases. If by sanitary condition is meant the social and economic condition of the people, then taxes, trusts, trades unions, the tariff and similar factors have far greater influence than any of the direct sanitary activities of the municipality.

There are a number of diseases, the mortality of which it is generally believed that modern medicine has done much to reduce. Among these are the accidents of child-birth. We are not surprised to see that deaths from child-birth have diminished three, but it is rather surprising to find that puerperal fever has shown an increase, even though it be only to the extent of one, particularly as the number of confinements has materially diminished. It is also surprising to find that septic infections have increased, though perhaps it may be due to greater accuracy in death returns. Diseases of the female genital organs caused six more deaths, notwithstanding the great number of ovarian custs and pus tubes that are successfully operated on. Perhaps, however, there is an actual increase of salpingitis, owing to an increase of gonorrhea. It may also be noted that deaths from tumor have remained stationary, notwithstanding that more accurate diagnosis has removed a large number of cases from this indefinite class. Appendicitis only appeared as a cause of death in 1890, yet it now causes 17 deaths per 100,000, although the surgeons save so many cases. Some of these deaths would doubtless formerly have been attributed to peritonitis, which has decreased four, but it is rather curious that appendicitis and other diseases of the intestines, together with peritonitis, have shown a net increase of 18. It is satisfactory to learn that deaths from erysipelas have decreased 17 and from hemorrhage 8.

If we glance over the list of diseases it will be seen that in those which exhibit the greatest decrease, the change has probably been due to more accurate diagnosis. This is certainly true in diseases of the brain, convulsions, hydrocephalus, debility, teething, old age, dropsy and unknown causes, a total of 278. Of these convulsions, hydrocephalus, debility and teething were almost exclusively reported in infants, and from them diarrheal diseases, diseases of the stomach and tuberculosis have been largely recruited, and to this cause is due the apparent increase of the latter. The other members of this group were usually certified to in old people, and the increase in kidney disease, heart disease, arterial disease, bronchitis, and definite diseases of brain and cord, are to some extent transfers from brain disease, old age, dropsy and the unknown group. In fact, the nature of the causes classed as unknown, at present 28 per 100,000, is probably entirely different from what it was fifty years ago.

Insanity shows an apparent decrease in Providence, but it is only apparent, and is due principally to the fact that many cases of insanity are sent to the State institution outside of the city, where they die. Also deaths which would formerly have been attributed to insanity are now attributed to arterial sclerosis, cerebral hemorrhage, kidney disease, or other underlying or intercurrent affections. Thus in Butler Hospital for the Insane, which is within the city limits, most of the deaths were formerly attributed to insanity, but now very few are. Hence our vital records tell us nothing as to whether insanity has or has not increased. The same is true of diseases of the nervous system in general. Besides insanity, brain diseases, including meningitis, paralysis, epilepsy and hydrocephalus, have decreased 94, and other diseases of the nervous system and apoplexy have increased 58, but there has been so much change in diagnosis that it is impossible to draw a satisfactory conclusion. Alcoholism has decreased 11 and I am satisfied that this is a real decrease.

Among the diseases showing an apparent increase there

are only a few in which it is certain that the increase is not to be explained by more accurate diagnosis, or changes in the age and sex distribution of the population. These are influenza, cerebro-spinal-meningitis, suicide, accidents and homicide. The deaths from suicide have doubled. Accidents have shown a notable increase, but mostly in that class due to new machinery or trades, such as electric car accidents, railroad and machinery accidents and poisoning by water gas. The homicides in Providence have more than doubled.

The number of deaths from syphilis is small, but has doubled, and is likely to be due to more accurate diagnosis.

As to whether cancer has increased except as may be explained by improved diagnosis, and changed age distribution, is not clear. That the former cause has operated to a considerable extent is shown by the fact that accessible cancer, as that of the breast, has not increased as fast as cancer of the internal organs. In fact, Newsholme has shown from carefully prepared European data, that it is almost certain accessible cancer has not increased at all. The proportion of the population at the cancer age has increased over 50 per cent. I have not as yet had time to study the death rate from cancer in all its details, but it would not be surprising if it showed no increase at all in the sense understood by laymen. Diseases of the heart and arteries, and especially diseases of the kidneys, have shown a very great apparent increase, but what has been said of cancer applies almost equally well to them. We do not know that it indicates any increased liability on the part of the individual.

The increase in bronchitis like that of pneumonia is doubtless due to changes in age constitution, improved diagnosis, and the influenza bacillus. Increase in diseases of the stomach is due probably to transference from convulsions and malnutrition, and the increase in premature birth to transference from debility. The small increase which occurs in several other diseases is probably also due to more accurate diagnosis.

CAUSES OF DEATH IN PROVIDENCE FOR FIFTY YEARS, 1856-1905.

The first column shows the average number of deaths for the whole period of fifty years.

The second column shows the increase or decrease between the first decade and the last decade of the fifty year period.

	Average Rate per 100,000 1856-1905	Increase or Decrease between 1856-'65 and 1896-1905		Average Rate per 100,000 1856-1905	Increase or Decrease between 1856-'65 and 1896-1905
Typhoid Fever	45	− 47	Diseases of Larynx	2	+ 1
Malaria	4	+ 4	Bronchitis	54	+ 49
Small Pox	1	− 7	Pneumonia	172	+ 51
Measles	14	+ 5	Respiratory Diseases	9	+ 1
Scarlet Fever	53	−114	Diseases of Mouth, etc.	2	− 4
Pertussis	21	− 8	Stomach	25	+ 23
Diphtheria and Croup	77	− 46	Diarrhœal Diseases	195	− 79
Influenza	15	+ 23	Hernia	4	+ 3
Erysipelas	7	− 17	Diseases of Intestines	13	+ 19
Septic Diseases	4	+ 4	Peritonitis	13	− 4
Phthisis	265	−169	Diseases of Liver	24	+ 5
Tubercular Diseases	36	+ 40	" " Kidneys	79	+127
Scrofula	3	− 20	Urinary Diseases	8	+ 7
Syphilis	5	+ 2	Diseases of Females	4	+ 6
Cancer	62	+ 41	Puerperal Fever	8	+ 1
Tumors	7	0	Child Birth	11	− 3
Rheumatism	7	0	Gangrene	3	0
Diabetes	8	+ 9	Abscess	5	− 1
Anemia, Etc.	4	0	Skin Diseases	2	− 3
Alcoholism	12	− 11	Organs of Locomotion	3	+ 3
General Diseases	2	+ 1	Hydrocephalus	19	− 55
Cerebro-Spinal Meningitis	6	+ 8	Malformations	12	+ 5
Brain Diseases	58	− 31	Debility	46	− 21
Cerebral Hemorrhage	68	+ 52	Premature Birth	26	+ 34
Paralysis	18	− 7	Teething	8	− 21
Insanity	11	− 10	Old Age	57	− 44
Convulsions	36	− 39	Suicide	9	+ 6
Epilepsy	5	− 1	Insolation	2	+ 4
Tetanus	1	0	Accidents	69	+ 22
Diseases of Nervous Syst.	8	+ 6	Homicide	2	+ 1
" " Heart	128	+ 78	Dropsy	5	− 21
" " Arteries, etc.	7	+ 19	Unknown	31	− 46
Hemorrhage	9	− 8			

These are a few of the thoughts that have come to me as I have been studying the death returns in Providence, and when Dr. Durgin asked me to read a paper this afternoon, I thought I would like to present them here, chiefly in order that I might get suggestions from others as to a further study and analysis of these factors. I should be very glad to have any suggestions.

DR. HARTWELL: Dr. Durgin called me up by telephone the other day, and told me of this meeting this afternoon. I ought to have known all about it, but I have only vague recollection of receiving an envelope with the imprint of the Association upon it. Taking it for a bill, which could be attended to any time within thirty days, I did not open it. Consequently, when Dr. Durgin asked me to open the discussion on Dr. Chapin's paper on the "Death Rates of Providence for Fifty Years," I supposed crude death rates were meant. I am glad, however, that the subject of the paper is "Causes of Death in Providence," partly because I shall more easily limit what I have to say, and more especially because causes of death is a very much more interesting and instructive topic than crude death rates.

After Dr. Chapin's remarks on his table it does not need to be interpreted. I shall not linger upon that or dwell upon the points which Dr. Chapin has so well brought out. As I see it, the main lesson for those who study and those who make vital statistics, conveyed by the paper and the table, may be summed up in "Go thou and do likewise." He has confined his attention to the material gathered in a city that has grown within the memory of many fourfold, from a town of 45,000 to a city of nearly 200,000 inhabitants. In this State there are many cities that have grown at a like rate, but not very many that have reached the same proportions. I hardly think that very many of our cities in Massachusetts have such a series of tables as constitutes the material from which Dr. Chapin has made his table. Nevertheless, we are at least approaching the point where the vital statistics of

the cities of Massachusetts contain a mass of data that ought to be digested, decomposed, if you like, and recomposed in accordance with the methods of modern statistical science.

Dr. Chapin's paper has one feature which cannot be too highly commended, it seems to me. He has confined his attention to the statistics of his own city, and has not indulged in the slightest comparison with any other city. That means much—more than is at first apparent, perhaps. A low death rate has come to be considered a good and necessary asset in many cities, and so a low death rate has been figured into existence, I venture to say, in an undue number of American cities within the last fifteen years. I cannot see what sanitary or other work has been done that will account for such startling declines in the death rate, as are frequently claimed in the reports of some hustling health officers, particularly in the West. Let us study the crude death rates in our own localities for a series of years; split them up where we can into death rates from special causes, especially for different age-groups, taking account of occupation and environment when it is possible, then shall our vital statistics become more serviceable and shed much more light on the condition of the public health than they have hitherto.

It is to be hoped that the final tables which Dr. Chapin spoke of will set forth those diseases that are most susceptible of prevention and regulation, death rates by age groups and sex, e. g., the age group, ten to fifteen, or five to fifteen. In the interval between ten and fifteen death rates, at least crude death rates, are lower than in any other period of like duration in human life.

Dr. Chapin alluded to final tables in which the causes of death in Providence will be subjected to more detailed analysis than is shown in the table before us. It is to be hoped that he will find it practicable to determine, for a series of years, the mortality rates for the more significant diseases, especially those that are measurably capable of prevention or regulation, by sex and age groups. Crude death rates are lowest for persons between ten and fifteen years of age.

Hence, even the crude death rate from the age group five to fifteen (since its members are least susceptible to lethal influences), would serve us as a useful criterion of the health of a community. I have shown that during the period of accelerated growth in height and weight, i.e., in the age period, ten to fifteen, the death rates of Boston children reach their minimum, but that the year of lowest death rate for girls falls one or two years earlier than for boys. How far this is true as respects special death rates remains to be determined. It is to be hoped that Dr. Chapin's paper can be utilized towards settling the question, and be made to yield tables showing death rates by individual years of age, i. e., 0—21 years—for each sex, for certain diseases, in census years, e. g., the developmental diseases, so called. I specify census years since it is only in those years that we have any approach to an accurate estimate of composition of the population by ages.

In the present state of the art of making tables of death-rates, especially for purposes of comparison between different States and cities, it is best to leave post-censal years out of account, as there is much diversity of practices, or rather an almost total lack of system in estimating population in such years, there is urgent need that municipal registrars and health boards should correct their estimates of population for inter-censal years for the purpose of correcting their published death rates for the past fifty years.

In this connection I may say that I have in preparation a table to show the corrected death rates of the city of Boston from 1800 to 1905, in which the observed increase of population from census to census is used as the basis for determining the true estimate of population for inter-censal years. Such corrected death rates are a desideratum, if a period of long duration is under consideration, owing to the inevitable fluctuations in the rate of growth in population. For instance, the population of Boston increased 12.9 per cent. in the period 1895-1900, and only 6.2 per cent. in the period 1900-1905. In other words, if the rate of increase in Boston's

population which actually did obtain between 1890 and 1900 had obtained between 1900 and 1905, we should have had 30,000 more people in Boston last May than were found by the enumerators. Naturally, the published death rates since 1900 have been computed on the earlier and larger rate of increase. As in Boston, so in Massachusetts, the rate of increase of population has varied for half decade to half decade since 1870, with no approach to uniformity.

There is a widespread and mischievous tendency to indulge in per capita comparisons of cities as regards death rates, pauperism, debt, etc., etc.; but such comparisons are for the most, misleading, not to say worthless, because of wild estimates of population in other than census years. For instance, the Health Officer of Cincinnati figured out a death rate of 13.74 per thousand for that city in 1897, on an estimated population of 405,000 against an estimate of 350,000 for 1896. He got his 405,000 population by multiplying the number of voters registered at the presidential election of 1896 by five, which he assumed as "a usual ratio." The census enumerators found only 325,902 persons in Cincinnati in 1900, the official death rates of that city were from 3.66 to 3.81 in the thousand too low for the years 1897-1899. Again, the death rate of Baltimore in each of the three years, 1898-1900, was figured on an estimated population of 541,000, though the population in 1900 as shown by the United States Census was only 508,957. At the same time a writer in the "Charities Review," in 1898, indulged in per capita comparisons relating to relief of the poor to the manifest advantage of Baltimore, since he credited Baltimore with 625,270 inhabitants in 1898, on the strength of "an official estimate."

In other branches of vital statistics, e. g., birth rates, the situation is even worse. Some years ago I computed the mean death rate and mean birth rate for the year 1901 for the ten largest cities in the country—using the figures given in an official publication of one of the departments of the United States Government. For an estimated population of upwards of 9,000,000, the birth rate, viz., 18.99 exceeded the

death rate by only 1.14 per thousand, indicating an excess of births over deaths of only 11.185 for the ten leading cities of the country taken together. Three out of the ten, viz., Baltimore, San Francisco and Cincinnati, showed without a tremor, without a symptom of apology, a total of 4.878 deaths more than births!

Clearly there is opportunity for much honest, careful, scientific work on the part of local registrars and official compilers of vital statistics. As a rule, the records of deaths appear to be more nearly correct than those of births, and are more promptly made than the returns of marriages. There is opportunity in this field for a campaign of education by boards of health for the purpose of awakening a keener sense of duty among physicians and clergymen as regards making their returns of births and marriages more full and prompt.

I welcome Dr. Chapin's paper as one of a new sort, that we ought to have more of. I hope that in the course of the next few years we shall have similar studies of the causes of death in individual cities and towns of Massachusetts. For such studies Dr. Chapin's paper may well serve as our model.

VICE-PRESIDENT CHAPIN: If there are no other remarks the meeting is now adjourned.

MUNICIPAL SANITATION.

By Charles V. Chapin, M. D.

Superintendent of Health, Providence, R. I.

GARBAGE REDUCTION. The garbage of New Bedford is now being treated in a new form of reduction apparatus. The works were designed and built by the American Extractor Company of Providence. They were completed last summer and since then practically all the garbage of New Bedford has been there disposed of. The capacity of the

works is sufficient for handling a great deal more garbage than is delivered. The most notable features of the plant, and those which distinguish it from other reduction works are the general excellence of construction, and the contrivance for expressing the water from the digested garbage. The garbage is dumped onto a concrete platform in a separate building where it is allowed to drain, and is shoveled once or twice and all the tin cans, stones, pieces of iron, etc., picked out, when it is finally thrown onto a conveyor which carries it automatically into the digestors. This part of the process might give rise to nuisance if the building was not well built and ventilated, and carefully looked after, as it is in New Bedford. The digestors extend up two stories, are eight feet in diameter and made of two-inch cast manganese iron. The garbage is digested for a number of hours, and the grease which rises to the top, is forced over into tanks, where it is separated from the water, and is finally drawn off into barrels. The garbage, now boiled into a pulpy mass, called tankage, is drawn off through a valve from the bottom of the digester, and passes through a peculiar press, which is continuous in its action. It is really a great worm, into one end of which the wet tankage falls and is gradually forced through by the motion of the worm, the water at the same time being expressed. A somewhat similar apparatus is used in paper mills, but has been much modified for this work on account of the very heterogeneous character of garbage tankage. It was said that only 50 per cent. of moisture was left in the tankage, and it certainly appeared to be as dry as that. This is a most excellent result. The tankage then drops into a dryer on the ground floor, from which it comes out dry enough to be sold to the fertilizer manufacturers. The apparatus is constructed so as to be continuous throughout, and the material when once thrown onto the conveyor, is not again exposed to the air until the dry tankage and the grease are discharged. The vapors are constantly drawn out of the apparatus and condensed with sufficient water to render them innocuous, and the gases are

consumed in the fires. The works are well built and are said to have cost $75,000. The machinery seems to be substantial and much care has been given to the details of construction. There is no reason why the works should not be operated without any nuisance, and they were so operated at the time of the writer's visit in November.

TEMPERANCE PLACARDS. Although it is well recognized that the abuse of alcohol is an important factor in the causation of sickness and death, American health officers have held aloof from temperance work. In England the matter is being seriously considered by sanitary officials. Dr. Niven, the medical officer of health of Manchester, not only believes, as we all do, that the abuse of alcohol predisposes to tuberculosis, but he thinks that the public house is the source of a great deal of tuberculous infection. The facilities for the transference of sputum in such places are abundant, and he is desirous of having them conducted in a more cleanly manner. Dr. Priestly* of Lambeth has had placards printed, setting forth the dangers to health from the use of alcohol, and has posted them in shop windows and on fences and the walls of buildings. This has also been done in several other London boroughs, and in some provincial cities. Apparently these placards were considered by the liquor dealers to be effective and therefore a danger to their business, for the South London public houses were not long afterwards placarded with notices insisting upon the virtue of alcohol as a preventive against consumption.

PREVENTION OF NOISE. The Board of Health of Fitchburg has a large placard bearing the legend, "KEEP QUIET, THERE IS SICKNESS IN THIS HOUSE." All persons are forbidden making any noise or disturbance in this neighborhood." The day before the Fourth of July this is placed on houses whose occupants ask for it, provided the board believes that the sickness is grave enough to demand this help, and the police are notified to see that the order is

*Public Health, XVIII, 1905-06, pp. 165, 221.

obeyed. It is obeyed to the great satisfaction of the neighborhood.

SANITARY ENGINEERING NOTES.

By Robert Spurr Weston, Asso. M. Am. Soc. C. E.

VARIABLE VIRULENCE OF TYPHOID FEVER. Professor Mason of Troy, N. Y., in a recent number of the New England Water Works Association Journal, pointed out that epidemics of typhoid fever spread by water from polluted wells had higher mortality than epidemics caused by drinking water from polluted rivers, owing to the decrease in toxicity of the bacteria during their struggle for existence in the stream. He also called attention to the fact that the death rate was lower during a severe epidemic of typhoid than it was when there were a few cases in a city—probably owing to the fact that in the latter case many of the recovering cases were not reported.

In discussion Dr. Soper corroborated these opinions, but thought that cases were often reported during epidemics which were not typhoid. Waves of these enteric diseases frequently precede the typhoid epidemics and if faithfully reported might give sufficient warning of the trouble brewing.

SOFTENING PLANT AT OBERLIN, OHIO. In a recent number of the Journal of the New England Water Works Association, Mr. Gerrish describes the recently-constructed softening plant, where very good results have been obtained. This plant cost $12,000 and by its operation adds about two and one-half cents per thousand gallons to the cost of the water delivered to the consumer. The natural water has about ten grains of calcium carbonate and six grains of magnesium sulphate per gallon, which are reduced by this process to 2.0 and 0.6 grains per gallon, respectively. The purification is brought about by the addition of lime and soda. Mr. Gerrish points out that the reaction is a slow one

and long storage greatly increases the effectiveness of the plant. After six days standing little or no further precipitation occurs.

COPPER SULPHATE AS AN ALGACIDE. At the September meeting of the New England Water Works Association the use of copper as an algacide was treated by a symposium on the subject by different authorities, about sixteen different experts reading papers or taking part in the discussion. The results are interesting and are published in a special number of the Journal which will repay reading.

The facts brought out show the importance of knowing the volume of water to be treated, the species of plant to be destroyed, the temperature of the water and the kind of fish in the lake, because the resisting powers of the different organisms vary greatly under different circumstances.

Algae appear in periodic waves and the application of the destroying chemical must be also periodic. In order to destroy bacterial life at all times the supply of chemical would have to be continuous. This seems impracticable, although it might be of use in a sudden epidemic or in the treatment of sewage effluents.

Bacteria are thought to be more resistant to copper sulphate than are algae. The harmlessness of copper in small quantities was mentioned; also the fact that it disappears in combination with organisms, and if imbibed in this form was not believed by some of the speakers to cause trouble. Experiments show that in good, clean water the copper is precipitated in a much shorter time than if the water is highly colored, and as much as 50 per cent. of the copper added may be kept in solution for a long time.

NEW WATER WORKS AT THE ISTHMUS. A reservoir having a capacity of 400,000,000 gallons has been built for the city of Panama. Almost a mile of pipe leads from this reservoir to a 1,000,000-gallon distributing reservoir. This system has been in use since July, 1905, and the water has been very satisfactory.

Panama has also a sewerage system. This is on the combined principle and the outlet discharges directly into the bay.

SEWAGE DISPOSAL AT BERLIN, OHIO. The Engineering Record gives a description of the extension of the sewage disposal plant at Berlin, O. Two septic tanks, two sewage tanks, a pumping station and fourteen acres of intermittent sand filters have been constructed. The trouble caused by factory wastes was overcome by special treatment at each factory. The septic tanks have a capacity for a 24-hours' flow. The sewage is pumped from the sewage tanks to filter beds once each day.

POLLUTION OF SHELL FISH. Mr. George W. Fuller in a recent address before the U. S. Sanitary Association, describes several methods of purifying sewage on the Atlantic seaboard. He stated that in England results show that from 8 to 50 per cent. of the typhoid fever cases in coast towns and cities are believed to have been caused by shell fish. Mr. Fuller believes that the value of the fisheries must be taken into account when sewage purification systems are proposed.

SEWAGE DISPOSAL AT OBERLIN, OHIO. The experiment of adding lime and iron sulphate to sewage has been tried at Oberlin, Ohio, in order to increase the precipitation of solids at the sewage farm. The wisdom of this process has not yet been determined upon.

LABORATORY NOTES.

By Frederic P. Gorman.

Associate Professor of Biology, Brown University, Bacteriologist, Health Department, Providence.

THE ROLE OF THE LEUCOCYTE. The chief evidence that the leucocyte plays a part in immunity is the fact that these cells have been observed to engulf and destroy bac-

teria. Recently it has been found possible to bring this property of the leucocytes under laboratory observation in a way that enables one to measure the phagocytic power of the blood in different individuals or in the same individual at different times. It may be that future experiments may develop a method of increasing this property of the leucocytes and so increase the immunity of an individual. In a paper read before the recent meeting of the Society of American Bacteriologists at Ann Arbor, Mich., Doctors McFarland and L'Engle of Philadelphia described their technique for the determination of this property of leucocytes. The method was originally devised by Leishman and modified by Wright and Douglas* and still further modified by the authors. The blood to be tested is mixed with a uniform suspension of a twenty-four-hour culture of Staphylococcus pyogenes aureus. Smears made from the mixture are stained with Marino's stain.** The average number of bacteria found within the leucocytes is taken as the index of opsonic power. The authors examined the blood of fifteen presumably normal individuals and found that the phagocytic index varied from 23.125 to 4.35. In two cases in which the counts were repeated at intervals of five days there was a remarkable uniformity in one case and a distinct variation in the other. The experiments were all performed between the hours of three and five in the afternoon. They also found that the number of bacteria taken up by the leucocytes varies with great regularity according to the strength of the bacterial suspension used. Hence, we have endeavored to use a uniform suspension in all of our experiments. The clinical bearing of these experiments is indicated by the fact that those individuals whose phagocytic index was lowest had suffered from carbuncles or boils or become easily infected from slight causes.

They conclude as follows:

"1. Leishman's methods of determining the phagocytic power of the blood, as modified by Wright and Douglas and by ourselves, is a very simple method adapted to clinical application.

*Proc. Roy. Soc. Lond. LXXIII, 1904, 128.

**Ann. Past. Inst. XVIII, 1904, 761 and XIX, 1905, 351.

2. Marino's stain is most appropriate for demonstrating the leucocytes and the contained bacteria.

3. There is no uniformity in the phagocytic indices of the bloods of supposedly healthy individuals.

4. The phagocytic index of the same individual may be constant or it may vary upon different days.

5. An exceptionally low phagocytic count usually indicates a present or past predisposition to suppuration (the phagocytic index in this regard being determined by the use of Staphylococcus pyogenes aureus).

6. The phagocytic index may not be below the average in all cases in which there has been a tendency to suppuration."

LEUCOCYTES IN MILK. In this connection also the question of the meaning of leucocytes in milk is important. Is the number present a measure of the health of the cow and can a determination of the number present be of any assistance to the sanitarian? These questions are at present under discussion and meanwhile the methods of determining the number are receiving a searching examination. The first method proposed was that of Dr. Stokes of Baltimore. Ten cubic centimeters of milk were centrifuged for five minutes. The cream and milk were then poured off, allowing the sediment to run to the bottom of the tube. The tube was then allowed to stand for an instant until a few drops had gathered at the bottom, when the contents were stirred up with a platinum loop, and a loopful was then spread on a coverglass, dried, flamed, and stained with methylene blue. Under the one-twelfth objective a number of fields were counted, and the average taken as the leucocyte count.

The next modification of this method was that of Doane and Buckley of the Maryland Agricultural Experiment Station.* Ten cubic centimeters of milk are centrifuged for four minutes in graduated sendimentation tubes, at an approximate speed of 2000 revolutions per minute. The cream is lifted out with a cotton swab, as much fat being removed as possible. It is then centrifuged for one minute more, and

*Maryland Agriculture Experiment Station Bulletin No. 102.

the cream again removed. The milk above any sediment which may have collected is removed with a small siphon to about the 1-2 c.c mark. Two drops of a saturated alcoholic solution of methylene blue are then added, thorougholy mixed with the sediment by shaking, and then set in boiling water for two or three minutes. Water is added to the one c. c. mark. The leucocytes in the solution are counted by means of an ordinary blood-counting apparatus and the number per cubic centimeter is then estimated.

A comparison of these methods shows that there is much to be desired in both as far as uniform results are concerned. If it is of any value to know accurately how many leucocytes are in a given sample of milk, other more exact methods must be adopted.

THE DIRECT MICROSCOPIC EXAMINATION OF MILK FOR BACTERIA. Dr. Francis H. Slack of the Boston Board of Health Laboratory has recently advocated* the direct microscopic examination of the sediment from a sample of milk, for the determination of the number and morphology of the bacteria present, as well as for leucocytes and streptococci.

The special apparatus used for centrifugalizing the milk samples consists of an aluminum disc and cover ten inches in diameter and five-eighth inch in depth, fitted to hold twenty small glass tubes, arranged radically. These tubes hold about two cubic centimeters each and are closed at both ends with rubber stoppers.

The samples in the tubes are centrifugalized ten minutes at a speed of 2000 to 3000 revolutions a minute, thus collecting the whole sediment from each sample on the outer stopper.

The sediment is obtained by breaking up the cream, pouring out the milk and carefully removing the stopper with the adhering sediment, not allowing any milk to run back on the sediment to disturb it. It is then evenly smeared with a drop of sterile water over a space of 4 sq. cm. on a glass slide, dried and stained with methylene blue.

*Ann Arbor Meeting of the Society of American Bacteriologists.

The number of bacteria found in a representative one-twelfth oil immersion field bears a fairly constant relation to the 1-10,000 plate culture (grown for twenty-four hours in a saturated atmosphere at 37 degrees C., 1 per cent. agar being used with a reaction of +1.5).

Thus, as a rough estimate, each coccus, bacillus, diplococcus or chain in a representative one-twelfth oil immersion field represents 10,000 bacteria to a cubic centimeter in the sample of milk examined.

Advantages are: rapidity of examination, accuracy, easily-learned technique, lack of costly apparatus.

Dr. Slack believes that the method can, in experienced hands, safely be used for certifying milk, certifying those samples in which no bacteria are found, since the large number of samples which could be examined and the increased efficiency of the supervision more than compensates for a slightly greater accuracy in plate counts.

ISOLATION OF TRYPANOSOMES. In view of the increasing importance of a study of the Protozoa infecting man and the lower animals any suggestions as to technique are important. In connection with a most interesting discussion and demonstration of their work with trypanosomes, Doctors Novy and Knapp, at the Ann Arbor meeting of the Society of American Bacteriologists, described their methods of isolating trypanosomes from accompanying bacteria. The same or a similar method might be used with other protozoa.

In general, it may be said that bacteria once introduced into a culture of trypanosomes tend to outgrow and check the development of the flagellates. In exceptional instances, however, the bacteria thus introduced exert little or no interference and may be even apparently beneficial. While in the former case the trypanosomes die out, in the latter instance the mixed culture may be kept for six months longer.

The isolation of the trypanosomes in pure form from such mixed cultures is a matter of some importance, especially when it is desired to study the pathogenic action of the flag-

ellates. The need of some method of separation was particularly felt in connection with the study of the mosquito trypanosomes which, since they are present in the intestinal canal, are always accompanied by various bacteria and yeasts. After many ineffectual attempts the following method was successfully employed for the isolation of pure cultures of herpetomonas and crithidia.

By means of a small glass spatula, made by drawing out the end of a glass rod, a little of the mixed culture was spread in a series of streaks over six Petri dishes containing solidified blood agar. The Petri dish, known as the "Kriegsministeriums-Modell 2," made by Greiner and Friedrichs, is particularly adapted for this purpose, inasmuch as it can be sealed effectually by means of a wide rubber band. The sealed dishes are then set aside at room temperature for ten to twelve days. The last plate or two of the series will be found to show isolated colonies of trypanosomes which can be transplanted in the usual way to the test-tube. This method will undoubtedly be found useful in future studies of the flagellates found in the intestinal canal of insects and other sanguivora. The intestinal contents can be spread directly over the plates in the manner indicated.

THE VOGES-PROSKAUER REACTION AGAIN. In connection with the discussion of the Voges-Proskauer reaction in the last number of this Journal it might be added that Harris of the University of Chicago has recently tested this reaction very carefully and reports as follows:*

The red coloration at times met with in fermentation tubes after testing the gas composition with KOH solution, was first described by Voges and Proskauer in 1898 as occurring in fermentation tube cultures of certain members of the hemorrhagic septicaemia group of bacteria, and the reaction was advocated by them as a test for differentiating members of this group. However, as proposed by them, the test is without value, because their observations were made on ba-

*Ann Arbor Meeting of the Society of American Bacterioligists.

cilli which we now clearly recognize as belonging to the hog cholera group, not to the hemorrhagic septicaemia group.

The color reaction has since been proposed by Durham and MacConkey as a differential test whereby B. lactis aerogenes, B. mucosus capsulatus (Pfeiffer), and B. cloacae and bacteria of such types may be distinguished from certain intestinal and other forms of bacteria. Howe and MacConkey likewise recommend it as possessing value in the bacteriological analysis of water.

Contrary to the views of the foregoing observers, the writer finds that the reaction occurs irregularly with such bacteria as B. coli, B. lactis aerogenes, B. mucosus capsulatus (var. Pfeiffer, and Friedlander), B. cholerae suis, B. enteritidis, B. icteroides, B. paratyphosus (several races), B. proteus vulgaris, B. cloacœ, B. fæcalis aligenes and B. typhosus and Streptococcus pyogenes.

Conclusions:

1. By Voges and Proskauer the reaction was obtained not with any member of the hemorrhagic septicaemia group of bacteria, but with one of the hog cholera group.

2. The writer finds that the reaction is not confined to any one particular group of bacteria, as found by Durham and MacConkey, but occurs widespread and irregularly amongst bacterial species.

3. As a differential test, the Voges-Proskauer reaction is of little value.

4. Its nature is at present unknown.

STANDARDIZATION OF ANTITETANIC SERUM. At the recent meeting of the Society of American Bacteriologists a committee was appointed, consisting of Doctors Kinyoun, Houghton, Park, Theobald Smith, Pease, McFarland and Rosenau, to take the matter of standardization of antitetanic serum into consideration, and to confer with other committees having this matter in hand, and to report at the next meeting of the Society. This committee was the result of a discussion following the reading of a paper on this sub-

ject by Dr. E. M. Houghton. Dr. Houghton's conclusions are as follows:

"Experience has shown that the Ehrlich test for determining the strength of anti-diphtheric serum is very reliable, and has been adopted by the U. S. Department of Public Health and Marine Hospital Service as a method of standardizing this serum. Many methods are employed for measuring the strength of antitetanic serum, but none have been generally adopted in this country. The results of laboratory tests indicate that the sera found on the market vary enormously in strength, as tested by the modified Behring method. It would seem desirable that a method be adopted for testing antitetanic serum similar to that in use for determining the strength of anti-diphtheric serum, but it seems to the writers that the test animals should be guinea-pigs and that the units of strength should be such that a curative dose of ten cubic centimeters of antitetanic serum would contain approximately the same number of units as the curative dose of anti-diphtheric serum, as recommended by the U. S. Pharmacopeia."

ISOLATING PNEUMOCOCCI FROM MIXED CULTURES. Starting with Hiss's demonstration that pneumococci ferment inulin while streptococci fail to ferment it, Ruediger* has prepared a blue litmus inulin-agar medium in which the pneumococci form red colonies. This medium is composed of sugar-free agar with the addition of litmus and inulin and is prepared as follows:

(a) Peptone (Witte), 10; agar, 15; sugar-free beef broth (neutral), 1000. Dissolve by boiling one hour, adding water from time to time. Heat in the autoclave for fifteen to twenty minutes (to prevent subsequent precipitation while sterilizing), clarify with egg and filter through cotton, making the volume up to 800 c. c. with distilled water.

(b) Dissolve 15 grams of pure inulin in 200 c. c. of boiling distilled water and add this solution to (a). Now add 20 c. c. of a five per cent. solution of litmus (Merck's highest purity)

*Journal of Infectious Diseases, January, 1906.

and tube, putting 7 to 8 c. c. of medium into each tube. Sterilize in the autoclave under ten pounds of pressure for fifteen minutes. As some pneumococci do not grow well in this medium it is necessary to add 1 c. c. of heated ascites fluid or serum to each tube of melted agar (which has been cooled to 45 degrees), immediately before using. In this mixture the pneumococci grow well and form red colonies in twenty-four to seventy-two hours.

Ruediger has shown further that pneumococci are practically the only mouth bacteria that ferment inulin. It is not fermented by streptococci (Hiss), staphylococci, pseudodiphtheria bacilli, Micrococcus catarrhalis, Micrococcus tetragenus and Bacillus mucosus. Among ten cultures of diphtheria bacilli was found, one which fermented inulin. Twenty-two cultures which were made from red colonies in plates that had been inoculated with material from throats of pneumonia and scarlet fever patients were studied in detail. All are Gram-positive cocci which grow chiefly in pairs on blood agar slants, but some also form chains in liquid media. All ferment inulin and all but four form green colonies in blood agar plates. Capsules could be demonstrated on more than half of the cultures.

PREVENTIVE THERAPEUTICS.

By Herbert D. Pease.

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STANDARDIZATION OF TETANUS ANTITOXIN.

At the recent meeting of the Society of American Bacteriologists held at Ann Arbor, Mich., a committee of that Society was appointed to consider the formulation of a standard test for estimating the strength of tetanus antitoxin. This committee was instructed to co-operate with any other committee or institution considering the subject. The membership consists of Dr. J. J. Kinyoun, chairman, and Doctors E. M.

Houghton, Joseph McFarland, W. H. Park, H. D. Pease, M. J. Rosenau and Theobald Smith.

INTRAMUSCULAR INJECTIONS OF ANTITOXINS. Meltzer and Auer* show that the absorption of certain substances at least, such as adrenalin, curare and morphine, takes place far more rapidly when they are injected directly into muscular tissue than into the subcutaneous tissues.

While the effect of these injected substances was not produced quite as rapidly by intramuscular as by intravenous injections, the difference in time between these methods was not nearly so great as between the intramuscular and the subcutaneous.

These results are undoubtedly applicable to other substances, such as antitoxic serums, and as the time element in beginning the treatment of diphtheria and tetanus is of the utmost importance it is strongly recommended that antitoxin be injected directly into muscles rather than subcutaneously. This latter method frequently results in the injection being made into adipose tissue, from which absorption is exceedingly slow. The intramuscular injections are not more painful, but, on the other hand, are, as a rule, much less disturbing than the more superficial.

ANTITOXIN TREATMENT OF DIPHTHERIA IN NEW YORK CITY. Billings† has summarized the results of the use of the diphtheria antitoxin prepared by the New York City Health Department Laboratory for ten years. His tables show that the mortality from diphtheria has been reduced 200 per cent., and the number of deaths in proportion to the total inhabitants 100 per cent. The latter he considers the more reliable index of the actual reduction in the mortality due to antitoxin treatment.

Moreover the results during the last three years are far better than those obtained during the first three years of the use of antitoxin, the number of deaths being 20 per cent. less in the former, although the number of cases was doubled.

*Meltzer and Auer. Journal Experimental Medicine, 1905, 7, 59.
†Billings. New York Medical Journal, 1905, 82, 1310.

Recently, 90 per cent. of the laryngeal cases has been saved, and the number of cases of the laryngeal type of the disease has been greatly lessened.

The mortality in the 10,557 cases (moribund cases excluded) treated by private physicians during the ten years was 8.3 per cent., while that for the 3547 cases treated by the Health Department inspectors was 4.0 per cent.

The day of the disease on which the first dose of antitoxin was given by the city inspectors is shown in tabulated form.

DAY OF DISEASES.

	1st	2nd	3rd	4th	5th	Over	Not Stated	Totals
Cases	503	1349	1035	380	141	178	32	3618
Deaths	8	38	48	58	28	33	1	214
Mortality	1.5%	2.8%	4.6%	15.2%	19.8%	18.5%	3.1%	5.9%

This is a most striking demonstration of the value and necessity for the early administration of antitoxin. The total mortality in 18,866 cases was only 7.3 per cent.

Immunizing doses were given to over 46,000 persons by the inspectors, and nearly 6000 by physicians. Of these only 120 (less than .25 per cent.) developed diphtheria after the first twenty-four hours and before the end of thirty days.

THERAPEUTIC VALUE OF ANTITOXIC ELEMENT IN SERUM. Cruveilhier,** in company with other French writers and clinicians, believes that all the valuable therapeutic elements in diphtheria antitoxin are not represented by the tests showing the number of antitoxic units in the serum. His experiments made on guinea pigs, which received uniformly fatal doses of cultures of diphtheria bacilli followed by equal quantities of antitoxic serums of various strengths, showed that in many cases the weaker serums, so far as antitoxic units were concerned, gave as good curative results as the more concentrated serums. The same held good when the serums were used as prophylactic agents and injected prior to the fatal culture injections, that is, the

**Cruveilhier. Annales de L'Institut Pasteur, 1905, 19, 249.

serums of weaker unit strength were usually as efficient as the more concentrated serums in equal amounts.

ARTIFICIALLY CONCENTRATED DIPHTHERIA ANTITOXIN. Gibson* has succeeded in concentrating diphtheria antitoxin three or fourfold by the use of chemical procedures. The serum albumen and the water insoluble globulins and certain nucleo-proteids in antitoxin serum are separated from the so-called soluble globulins with which the antitoxic principle is identified or associated.

The final product sterilized by antiseptics and filtration is clear and colorless or slightly tinged with haemoglobin, and from Gibson's tests maintains its power as long as the original serum does. The concentrated serum has been used in New York city for several months and has given excellent results. A decreased tendency to the severer forms of the so-called serum sickness, skin eruptions, joint pains, etc., has been noticed, and this decrease, together with a lessening of the bulk of liquid to be injected, will tend to increase its popularity.

IMMUNIZATION THROUGH THE FEEDING OF MILK OF IMMUNIZED ANIMALS. Salge†, who had previously shown that antitoxin when fed to infants in the form of serum taken from animals did not pass into the circulation from the intestinal canal, but was so absorbed when present in the feedings of mothers' milk, endeavored to ascertain whether the antitoxin antibodies in the milk from goats artificially immunized to diphtheria and typhoid fever would be absorbed into the circulation of infants fed with such milk.

His results were altogether negative, the antitoxic and specific bactericidal substances were not absorbed from the infant's intestinal canal. It would appear from this work that the plan of feeding to infants the milk of cows artificially immunized to tuberculosis devised by Behring would be unsuccessful.

*Gibson. Journal of Biological Chemistry, 1906, 1, 161.

†Salge. Jahrbuch fur Kinderheilkunde, 1905, Band 11 (3rd Series) p. 486.

VETERINARY SANITATION.

By Veranus A. Moore, M. D.

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THE SERUM DIAGNOSIS OF GLANDERS. The doubt that still exists in the minds of many concerning the diagnosis of glanders with mallein, especially in horses that have been exposed, but which show no physical signs of the disease, renders the serum, or agglutination method a matter of unusual interest. The prevalence of glanders in many places, the value of the animals exposed, and the seriousness of the disease for both horses and men are sufficient to arouse interest in any method for making a positive diagnosis, as well as means for its eradication. The work recently published by Schutz and Miessner† is worthy of consideration.

The authors made extensive investigations to determine the value of the agglutination method in the diagnosis of glanders.

The experiments were made with glanders bacilli, which were killed by exposing them for two hours at a temperature of 60 degrees C., and then emulsified with a carbolized solution. To 2 c. cm. of this emulsion, the blood serum of the suspected animals was mixed in various quantities. The mixture was then placed in an incubator and kept at 37 degrees C. temperature for 24 to 30 hours. The results of these experiments were, that while the blood serum of horses free from glanders agglutinated the bacilli at the most in proportions of 1:400, those of glandered animals as a general rule produced a reaction in proportions of 1:1000 to 1:2000. The blood serum of horses artificially infected with a virulent culture gave a reaction beginning on the fifth to seventh day from the time of infection, which increased in the following four or five days (in one case to 1:8000), and then remained at that height for about one month. After that time, it gradually diminished (to 1:800 to 1:400). According to this, with the agglutination method an infection of glanders can be

†Archiv fur Wiss u. Prak. Thierheilkunde, August 1905.

determined which occurred eight to fourteen days previous to the test. From a practical standpoint this is of great value for retesting the horses in stables where already infected horses have been destroyed; the test is to be made two to three weeks after, and in this way the new infection can be determined by the reaction. Two experiments lead to the result that previously injected mallein has no effect on the agglutination. The blood serum of horses free from glanders has not agglutinated the cultures in higher proportions than 1.800, while in those affected with pneumonia or pleurisy the proportion even reached 1.1000. In the Pathological Institution of Berlin, in two years, the blood serum of 2,209 horses has been examined with the following results:

Of 1,911 horses free from glanders, in 1,232 or 64.8 per cent. the blood serum agglutinated 1:100—1:300; in 363, or 19.0 per cent. the blood serum agglutinated 1:400; in 135, or 7.1 per cent. the blood serum agglutinated 1:500; in 123, or 6.4 per cent. the blood serum agglutinated 1:600; in 41, or 2.2 per cent. the blood serum agglutinated 1:800; in 11, or 0.5 per cent. the blood serum agglutinated 1:1000; while a reaction higher than 1:1000 could not be observed in a single case.

Of 298 glandered horses, in 6, or 2 per cent. the blood serum agglutinated 1:400; in 12, or 4 per cent. the blood serum agglutinated 1:500; in 44, or 14.8 per cent. the blood serum agglutinated 1:600; in 47, or 15.8 per cent. the blood serum agglutinated 1:800; in 75, or 25.2 per cent. the blood serum agglutinated 1:1000; in 49, or 16.4 per cent. the blood serum agglutinated 1:1500; in 65, or 21.8 per cent. the blood serum agglutinated 1:2000.

These uncertainties in the agglutination are somewhat reduced by the fact that the smaller proportions in the agglutination are, as a general rule, in such horses as are affected with a severe and extensive form of the disease; further, that in glandered horses, the agglutination power of the blood serum is with the passing of time gradually diminished in the way that from the tenth to eleventh day following the infec-

tion, for a month it corresponds to 1.2000, then from two to three weeks it drops to 1:500 1:1000 1:800, 1:600, 1:500; while, on the other hand, in horses free from glanders, the agglutination power of the blood serum does not change. The authors applied the agglutination method in numerous cases in practice and based on their experience they recommend the following method for the diagnosis and eradication of glandered horses:

1. From 20 to 50 cubic centimetres of blood is taken from the glandered or suspected horse, the bottle is carefully marked and accompanied with the history and other data regarding the infection, etc., is sent to the experiment station.

2. All horses of which blood serum agglutinated in porportion of 1:1000 or higher should be destroyed.

3. The same way all horses should be destroyed of which the blood agglutinates only in proportions of 1:500—800, but otherwise show the clinical symptoms of glanders.

4. All other horses in which the agglutination occurs in dilutions of from 1:500 to 1:800, should be isolated and only destroyed in such cases, when justified, by a second test, at which the proportion of agglutination changed or diagnostic symptoms develop. If they do not react at the second test they may be pronounced free of glanders.

5. After establishing glanders, the blood of horses in the same stable should be tested after three weeks, and this should be repeated as often until the last two tests show in all horses individually a uniform reaction.

THE SPREAD OF BOVINE TUBERCULOSIS.—A very clear warning was sounded at the Eighth International Veterinary Congress in Budapest, last September, relative to the spread of tuberculosis in cattle. The feeling seems to exist that because it is a disease of slow development that it is not rapidly disseminated. Dr. Regner of Stockholm pointed out the fact that during the year 1904 tuberculosis had found its way into 52 dairies where it did not exist the year before, and that 5.9 per cent. of the cattle were infected. This

is an important observation, and one which should inspire renewed efforts to those entrusted with the control of tuberculosis in cattle in this country. The fact that in certain and quite widespread localities there is a large percentage of dairy cattle affected with tuberculosis is a sure indication that the disease is spreading rapidly to the uninfected animals.

THE PROPHYLACTIC VALUE OF TETANUS ANTITOXIN.—It is a well-established fact that tetanus in both man and animals is quite prevalent in certain localities. The loss of animals, especially horses, from this disease is, in these infected localities, very considerable. In order to overcome this, tetanus antitoxin is being used more and more as a prophylactic. When animals are injured in such a way that infection with tetanus is liable to occur, or when they are to be subjected to an operation, (such as castration,) they are treated with immunizing doses of tetanus antitoxin. The demand for tetanus antitoxin for this purpose is increasing very rapidly, especially in France. This is shown by the fact that in 1896 there were sent out from the Pasteur Institute of Paris 1,511 bottles containing 10 cc. each; in 1898, the number was 24,950, and in 1900 it exceeded 43,000 bottles. It is given hypodermically, in two doses, from 10 to 12 days apart. The immunizing dose for large animals is 20 cc., and for small ones from 6 to 10 cc. The method is being used with good results by many veterinarians in this country.

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